

Department of Genetics, University of Wisconsin
Madison 6, Wisconsin.

August 21, 1951.

Dear Dr. Nobel:

Thank you very much for your photographs, phage specimens, and letter of August 8, all of which arrived promptly and in good condition.

I was delighted to learn that you might visit the States this fall, and I certainly do hope you will be able to visit Madison. Unfortunately, we are about a thousand miles west of New York, and it will take some time here to make definite arrangements to underwrite your travelling expenses. I am quite confident that this will be possible. When you have formulated your travel plans, could you let me know whether you can include Madison in your itinerary, and if so, which dates would be most convenient for you. I have a great many questions I should like the opportunity to discuss with you, and hope you can manage to stay a couple of days, but it is obvious that there will be a great many demands on your time.

Your review in Bact. Revs. was very much appreciated, and I suspect will help a good deal to focus intelligent interest on the subject again. If you could spare as many as four reprints, we could make excellent use of them, in the classroom as well as the laboratory.

Efforts so far to correlate L-forms and genetic activity in *E. coli* have been completely unsuccessful so far. With B, I have not detected any form of growth in filtrates (after T2 or T3), either in broth or on serum agar. However, the appearance of the unfiltered partially turbid mixtures is unmistakable, but even these gave no interesting development on serum-penicillin agar. In a further test, biochemical mutants of B were used (P-, proline; and H-, histidine). Mixed cultures in the presence or absence of T3 gave no prototrophs on minimal agar, but, of course, there are very few bacterial survivors of the phage. I had some difficulty in obtaining T3-resistant mutants, for most surviving colonies are, *mirabile dictu*, quite sensitive to the phage. However, it did no help as far as detecting recombination goes, to use P- + H-/3 + T3, although such mixtures showed typical bacteria together with an abundance of the swollen and some granular forms. Until we have succeeded in obtaining consistent growth in the L-form, these experiments cannot be regarded as conclusive, but so far we have had no encouragement. Our insistence on filtration may be responsible for much of our difficulty. Is the regeneration of B from T3-filtrates a reproducible experiment? Are there any details of medium worth mentioning?

Difficulties with "204" are of another sort. The pleiomorphism is obvious and consistent, but I simply have not been able to secure any mutants that could be used for genetic tests. This stability may, of course, be far from fortuitous.

In *Salmonella*, critical evidence to identify "FA" with L-granules is still lacking, although FA-activity is still consistently associated with granules capable of swelling into large bodies with O-antisera. No further development of these bodies ~~has~~ has been observed.

Sincerely,

Joshua Lederberg

Is there any possibility of mailing an L-culture from *E. coli* B - or are they too fragile. Our failures to date have been somewhat discouraging.